

Genetics Department
University of Wisconsin
Madison, Wisconsin
April 28, 1954

Professor Hisao Uetake
Department of Microbiology
Sapporo Medical College
West 17, South 1
Sapporo, JAPAN

Dear Professor Uetake:

I have just received your letter and remarks of April 20 for which I am most grateful to you. I have been somewhat acquainted with experiments along these lines since receiving reprints from Iseki and Sakai of Gunma University. However I had found these publications quite difficult to understand by contrast with the lucid exposition of your letter.

phages
From your remarks, especially that the "changes of all antigenic structure are always associated with lysogenization" I would agree that this phenomenon has much more in common with Groman's findings than with ours on transduction. In any published account of your work, I hope attention will be given to documenting this statement with explicit experimental detail. There has been considerable confusion in the analysis of the diphtheria phenomenon owing to the failure of most workers up to very recent times to make this point entirely secure. Can you, for example, plate out the phage preparations so as to secure single ~~flax~~ and from these ~~flax~~ recover lysogenic bacteria that are invariably altered in their antigenic structure?

As to the differences between your observations and my own, the most important is of course the easy separation of transmission of any given character from the action of the phage, most readily by growing the phage on a series of different hosts. Our lysates could be prepared, by a number of methods however, without altering this principle.

I am not sure that I understood your point 3 that you had not yet obtained evidence of "replacement of genetic trait". Do you mean by this simply that no other characters other than the O antigen have been observed to be altered in your experiments?

For some time I have been discussing with Dr. P. R. Edwards the desirability of a confirmation of these experiments. Your letter serves as an additional stimulus to this end, and I will undertake to see what can be done about it. While we should be able to repeat these experiments with the use of stock strains of group E-1 and E-2, it would perhaps facilitate the conclusion of our experiments if we could obtain from you samples of the same bacterial cultures and bacteriophages that you have used in your experiments.

Finally, in the capacity of an associate editor, I wonder if I might not urge you to consider submitting your experimental findings in the form of a paper to the Journal of Bacteriology in this country. If you would be kind enough to send such a manuscript to me, and it conforms to the excellent style and content of your letter, I would have no hesitation in recommending its publication to the editor, Dr. Porter, despite the slight additional difficulty that is imposed by the factor of distance. Such a procedure would have the advantage of most readily spreading information as to your work to a large group of occidental research workers who might otherwise remain unaware of these findings for a much longer period of time.

I shall be looking forward to a continued exchange of information ~~on~~ on these topics of such great mutual interest.

Yours sincerely,